really originated there, and were not derived from the pollen."

Equally deplorable would be the result of affirming with Dr. Brown (p. 230) that "Turnip leaves contain 3 to 10 per cent. [of silica], oat 11 to 58 per cent. (especially in the stem), lettuce 20 per cent., oak-leaves 31 per cent., and beech-leaves 26 per cent."

It is unjust to the memory of Grew to assert that he ever disputed the discovery of the sexuality of flowering plants with Millington. Anyone who will refer to Grew's "Anatomy of Plants," p. 171, will see that he does perfect justice to Millington.

We had noted down a number of other passages equally open to criticism, but it is sincerely to be hoped in the interests of real botanical study that the specimens of this book which have been given will have some deterrent effect upon its possible readers. It is in vain that the author assures us that he has perused, for the purpose of his book, no less than 1,200 papers in almost every European language. A tithe of this literature properly selected and properly digested would have produced a manual of some value, instead of a mere chaotic dust-heap of all kinds of views belonging to all kinds of authors, as if scientific literature were in a way canonical, and the date of an author's views made no sort of difference, a common authenticity—like inspiration—embracing them all.

The blunders in the names of plants all through the book are quite as remarkable as the statements about their structure. *Chamœoparinus* (p. 101) is something more than a misprint for *Chamæcyparissus*, and it is astonishing to read about the "*Brownonian*" movements in a book whose author bears the honoured name of Robert Brown.

OUR BOOK SHELF

Telegraph and Travel. By Colonel Sir F. J. Goldsmid, C.B., K.C.S.I., &c. (London: Macmillan and Co., 1874.)

DURING the time of the late Bengal famine we were familiarised with seeing in the morning papers telegrams that had been despatched from Calcutta on the previous evening. Ten years ago telegraphic communication with India was but just completed *viâ* Constantinople, the Persian Gulf, and Karāchi: but it was some years after that before rapid through communication was arranged. The delays occurred mostly between Persia and England, and much organisation of European lines was needed before it was possible to converse with Teheran as the Shah did on his arrival at Buckingham Palace.

Those who are interested in the subject of telegraphic communication with our Indian Empire (and who is not?) will find much information in Sir F. J. Goldsmid's "Telegraph and Travel." He gives an account of the origin and development of the schemes, the troublesome diplomatic delays, and the physical difficulties that had to be overcome, as well as the arrangements that had to be made in some districts to protect the overland lines from destruction by wandering tribes. An officer of experience among Turks of Europe and Asia expressed his opinion at the outset that every convention with the Arabs in the interest of telegraph companies would be uncertain of execution, and that all wire within reach would be torn down from the poles to make heel-ropes for their horses. Instances of wilful damage unhappily were found by experience to be not rare, so that in some districts mounted guards were needed along wide tracts, adding, of course, considerably to the working cost of the lines.

The first part of the book the author feels is likely to be "found painfully practical and matter of fact, overburdened with official details and wanting in the zest which keeps the eye willingly open and the hand steady to the book," and he pleads in excuse "the necessarily monotonous character of the subject." The accomplishment of such a communication between the two countries, however, is so momentously important an event, that the history of its progress is of interest, however it is told. Sir F. J. Goldsmid's arrangement of his materials certainly does make it rather difficult to follow the thread of the history, but then it is enlivened with many interesting little sketches, descriptions of Persian diplomatists, their manner of conducting business, and so forth.

The first part of the book is illustrated with two maps which indicate the route of the different telegraphic lines between England and India, the dates being affixed to the different sections. Sir F. J. Goldsmid writes from his own experiences and from blue-books, and gives a mass of information which could not well be compiled by anyone not practically acquainted with the work.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Sir J. Herschel on the Endowment of Research

THE following extract from a letter from Sir John Herschel bears so directly on the distinction between the needs of theoretical and practical science insisted on in your recent leading article (vol. xi. p. 301), that I need offer no apology for communicating it. As the present value of the opinions which it expresses is intrinsic, it is unnecessary to particularise the circumstances under which the letter was written more than thirty years ago. But I may remark that it is supported by many passages in other letters in which the distinction in question, and that between research which *can* and research which *cannot* be readily effected by private means, is dwelt on (with all the scrupulous care of one than whom no responsible guardian of the public purse was ever more opposed to dependence on State aid as a principle), in a sense emphatically favourable to the demands of science for help in certain clearly indicated directions. I am sorry that I have not the papers at hand to quote from, but one instance in particular occurs to me, in which the extending and perfecting of various Physical Tables in a thoroughly satisfactory manner is declared to be altogether outside of the field of work of the individual investigator, and to be labour to be *paid for* by the community. J. H.

Biarritz, Feb. 22

". . . There is a remark which possibly it may be deemed presumptuous in me to make, relative to the general subject of scientific expenditure touched on [in your letter], but which I trust may be pardoned, as I have reason to believe my impressions on the subject are those of the whole body of British men of science, with hardly an exception. Large as the sum expended on objects officially classed as 'scientific' may appear it would not, I think, be considered as excessive if devoted to the prosecution of scientific objects in the highest and strictest sense of that word. I mean such as would be recommended for prosecution by men of science the most eminent, each in his several department, and responsible for their recommendations to the opinion of the public and of the scientific world. Under such objects I should certainly not include hydrographical, industrial, or military surveys, experiments merely technical, or many other objects, which, however indisputably necessary and

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requiring for their due execution scientific and refined processes and the superintendence of scientific men of high qualifications, are yet, properly speaking, rather applications of scientific views and acquired skill to particular objects of national importance, than undertakings of research having in view as their primary object the advancement of science itself. It is true, that as practice makes perfect, science *does* gain by such applications, and that by going somewhat out of the way in their execution, and seizing opportunities, most valuable theoretical results and data are occasionally elicited at an additional cost incomparably less than would be incurred by instituting operations for the purpose *ab initio*. But when I consider the pregnant nature of scientific truth, and how upon occasion of every well-grounded accession to, or extension of, theoretical knowledge, *a new practice* has arisen founded thereon, and old methods have been abandoned as *inefficient* and *uneconomical* in comparison, I should feel prepared to advocate or defend a very large and liberal devotion indeed of the public means to setting on foot undertakings, and maintaining establishments, in which the investigation of physical laws and data should be the avowed and primary object, and practical application the secondary, incidental, and collateral one.

"This, however, has hitherto been the fortunate lot of Astronomy only. And the result has been, *not only* the establishment of a complete theory—*not only* the perfection of nautical tables and observation—but an universal impulse given to every other branch of exact inquiry—a higher standard erected everywhere, a precision in every determination rendered practicable, which would have never before been dreamed of as attainable without the requirements of Astronomy. Is it hoping too much that the day may not be far distant when Physical Science in all its exacter branches shall participate in these advantages, and when the establishment of 'Physical Observatories' in our own and distant lands shall give that impulse to many other sciences (as for example Magnetism, Meteorology, &c.) of which they stand so much in need?" . . . . "J. F. W. H."

Trade Winds

MAURY, in his "Physical Geography of the Sea," maintains that the surface trade wind of the northern hemisphere becomes the upper counter current of the south, and *vice versâ*. That the trade winds, in fact, cross each other so—

FIG. 1.

instead of meeting and turning back over themselves so—

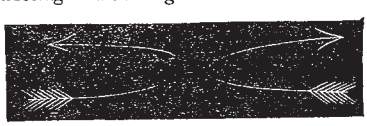

FIG. 2.

Subsequent writers on physical geography have repeated this statement without apparently reflecting on its extreme improbability.

Maury's arguments for this strange theory are partly connected with the hygrometric state of certain of these currents, partly with terrestrial magnetism, and partly with the nature of the air-dust. It would take up too much of your space to discuss these points fully. The arguments founded on terrestrial magnetism are, however, purely hypothetical and very fanciful. Those on the hygrometric state of the currents are not very convincing. It is, however, to the latter of Maury's arguments I wish to draw your readers' attention. Maury seems to believe in this almost incredible direction of the air currents because Ehrenberg identified certain South American infusorial forms in the red dust which often falls at sea near the West Coast of Africa and in South Europe. Did Ehrenberg simply identify certain South American forms in the dust, or did he identify the dust as South American on account of the presence of these forms? If the former, the argument goes for little; South American forms may be found in Africa also. If the latter, then a new difficulty arises. Every microscopist knows the curious diversity of infusorial forms in all climates at all similar. It would be the height of presumption even to question the conclusions of Ehrenberg in microscopy; and yet to be able to identify infusorial forms in such a way as to say that dust containing them comes from such and such a locality is certainly very wonderful.

Maury, from some of his remarks, does not seem to be fully alive to the utter inconsistency of his theory with what we know of the laws of fluid motion. That two broad flat rapid currents should encounter or flow into the same rising current and then cross through each other in alternate strips, or *curdles*, as Maury calls them, is scarcely within the bounds of physical possibility. On the other hand, Maury's opinions are certainly entitled to consideration, and this is one which he found with so much deliberation, and entertained so firmly, that I should gladly learn what competent physicists of the present day think of it.

Graeff Reinet College, Nov. 13 F. GUTHRIE

The Arctic Expedition

THE absence of sunlight during the Arctic winter is said to have an injurious effect on the health of both men and dogs; yet it does not appear that the best substitute for solar light has ever been employed for illuminating purposes during the dark season. It occurs to me that the occasional use of the electric-light would be likely to mitigate the evils due to the absence of solar radiation, and the constant use of oil lamps. If Gramme's electro-magnetic apparatus could be conveniently used on board ship, it would appear to offer the additional advantage of giving employment to the men at a time when it is difficult to find occupation for them.

Dublin, Feb. 23 R. J. MOSS

Herapath's Balance

CAN any of your readers inform me whether Herapath completed his balance, in which he suspended the beam from a magnet; also whether the idea was taken up by balance makers? He gives an account of this form of balance in a paper dated 1821. E. W. P.

OUR ASTRONOMICAL COLUMN

THE BINARY STAR μ^2 BOOTIS.—Dr. W. Doberck, of Col. Cooper's Observatory, Markree Castle, Sligo, has communicated to the Royal Irish Academy, and also published in *Ast. Nach.* No. 2026, an orbit of this binary founded upon a very complete discussion of the measures from 1782 when the duplicity was detected by Sir W. Herschel, to 1873. The resulting period of revolution is 290 years, and the true peri-astron passage is found to have occurred about 1863·5. Dr. Doberck does not append an ephemeris of angles and distances according to his orbit, but we supply them for the next eighteen months for comparison with any measures that may be made in the interval :—

1875·25	Angle	144°·79	Distance	0″·632
75·75	„	142 ·83	„	0 ·634
76·25	„	140 ·89	„	0 ·637
76·75	„	138 ·96	„	0 ·640

FALB'S NEW VARIABLE IN ORION.—The star to which reference was made in NATURE last week, appears to be the preceding component of the double star Σ 747, or that which was the smaller star during Struve's measures 1825-36. Herr Falb has given some particulars relating to this object in No. 2,026 of the *Astronomische Nachrichten*, but we suspect he has inadvertently reversed the order in which the magnitudes of the Dorpat Catalogue should be assigned. Struve's mean is

1833·59 Angle 223°·06 Distance 35″·85

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